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Comment on acp-2021-721

Jürgen Kesselmeier (Referee)

Referee comment on "Long-term fluxes of carbonyl sulfide and their seasonality and interannual variability in a boreal forest" by Timo Vesala et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-721-RC1, 2021

General View

I was glad to read this report about long-term series of measurements of carbonyl sulfide fluxes over boreal forests. This 5-years study delivers a large data set and a great overview about the seasonality and interannual variability. Furthermore, it greatly supports the process study-based knowledge that stomatal conductance is one of the keys to interpret the flux behavior under day and night conditions, though the final consumption of COS is light independent, based on the enzymatic degradation by several enzymes. The results of this strong project must be published. However, the current version can and should be improved. The weaknesses of the current parameterization should be discussed. This parameterization is only a first attempt to interpret the monthly fluxes and embed the measurement data into a wider description based on environmental factors, such as the measured photosynthetically active radiation, vapor pressure deficit, air temperature, and leaf area index. The procedure is valid to get an overview but this parameterization obviously still needs some fitting parameters. What does this fitting mean? Altogether, the fitted simulation matches the general exchange pattern over several seasons in a satisfactory manner. However, the total net flux is a result of a complex process which is affected by contributions of trees, cryptogamic covers and soils which may contribute significantly as it has been demonstrated earlier by several process-based studies (see review by Whelan et al. 2018). Here a complete overview about the environmental factors will be of help for the reader to understand.

Specific Requests

One of the most important products of this paper is the development of a parametrization for FCOS. The reader automatically searches for information under Material and Methods. However, there one finds only a short chapter about the fitting parameters. I propose to shift the description with the corresponding formulas from the Results-section to Material and Methods. Furthermore, the information that fitting parameter were retrieved with MATLAB is not really satisfying. I miss information about the basics, i.e. PAR, Ta, VPD, and

total LAI data over the 32 months. This information is needed to get a feeling for interannual and seasonal fluctuations. Why not producing a figure giving an overview about these environmental keys in a similar manner as for the fluxes in figure 1? Thus, the reader would get a picture about the seasonal microclimate.

Eddy correlation measurements deliver data for fluxes under turbulent conditions. I miss some critical remarks concerning nighttime flux data. As discussed by the authors, higher vegetation with a well-adapted gas exchange under stomatal regulation can be ruled out, though not completely. However, there can be a strong uptake by soils and cryptogams during the night which becomes strongly visible under a stable nocturnal boundary layer. The authors interpret non-zero uptake value in the dark simply as the ecosystem nocturnal COS uptake. Could this be specified and related to soil and cryptogams? The finding that the temperature is the most important factor governing FCOS at least gives room to have a closer look.

The simple semi-empirical parametrization, as the authors call it, is very helpful. However, I miss a holistic overview based on this promising long-term study at Hyytiälä. Which information can be given about the meteorological background, plant development and seasonal behavior? Where are reports about simultaneous measurements of exchanges with canopy and soils and the atmosphere? I expect this diversity is of special importance to handle flux measurements above a boreal forest which can present a quite open structure.

There are some disagreements between measured data and simulated ones as indicated in figure 1 for the years 2013 and 2014. This is only poorly addressed in the text. Are there any environmental factors to be made responsible? Meteorology? Plant development? See my comments above. A report about the environmental factors which are the basis for the parameterization will clearly help. Within this context, vapor pressure deficits not only affect higher plants with stomata but also the water content of cryptogamic tissues

The light saturation of FCOS (Fig. 2d) is interesting. How is this understood? The consumption of COS by the enzyme carbonic anhydrase is light independent and stomatal restriction under the given light intensities should not be expected. Besides carbonyl anhydrase, a contribution of the carboxylation enzymes phosphoenolpyruvate carboxylase and ribulose-1.5-bisphosphate carboxylase has been already reported earlier (Protoschill-Krebs and Kesselmeier, Bot. Acta 105, 1992, 206-212) and may help to understand a light saturation. Interesting to see this within flux data. Is this light saturation incorporated into the parameterization?

A remark concerning the numbers derived. Sandoval-Soto et al. (Biogeosciences, 2, 125–132, 2005) made the first attempt to recalculate the global budget for the COS vegetation sink based on GPP but corrected by measured COS/CO2 uptake ratios (experimentally obtained deposition velocities). They used older primary productivity data (Whittaker, R. H. and Likens, G. E.: The biosphere and man, in: The Primary Productivity of the Biosphere, edited by: Lieth, H. and Whittaker, R. H., Springer Verlag, New York, 305–328, 1975) to upscale COS sinks. This attempt demonstrated a clear underestimation

of the global vegetational sink at that time and initialized re-estimations. Within their data sets, they came to a number of 0.036-0.063 Tg [COS] per year for the uptake by boreal forests, which is equivalent to 19.2-33.6 Gg S per year. Interestingly, this number is ranging quite close to the 16.6 Gg as estimated by the authors on the current Hyytiälä data. It is interesting to see the developing numbers in view of all the uncertainties. Within this context, I would like to propose to change a principle of citation. In the first and second line on page 2 there is a number of papers cited to demonstrate the relationship between COS and gross carbon uptake. The choice looks arbitrary. I think it would be adequate better to skip all the citations and cite one recent review paper Whelan et al. (Biogeosciences, 15, 3625–3657, 2018) instead. The history and number of important contributions to this special topic are much larger and the proposed review is giving a more complete story.